

THURSDAY, APRIL 30, 1903.

RADIUM.

THE discovery by Monsieur and Madame Curie that a sample of radium gives out sufficient energy to melt half its weight of ice per hour has attracted attention to the question of the source from which the radium derives the energy necessary to maintain the radiation; this problem has been before us ever since the original discovery by Becquerel of the radiation from uranium. It has been suggested that the radium derives its energy from the air surrounding it, that the atoms of radium possess the faculty of abstracting the kinetic energy from the more rapidly moving air-molecules while they are able to retain their own energy when in collision with the slowly moving molecules of air. I cannot see, however, that even the possession of this property would explain the behaviour of radium; for imagine a portion of radium placed in a cavity in a block of ice; the ice around the radium gets melted; where does the energy for this come from? By the hypothesis there is no change in the energy of the air-radium system in the cavity, for the energy gained by the radium is lost by the air, while heat cannot flow into the cavity from outside, for the melted ice around the cavity is hotter than the ice surrounding it.

Another suggestion which has been made is that the air is traversed by a very penetrating kind of Becquerel radiation, and that it is the absorption of this radiation that gives the energy to the radium. We have direct evidence of the existence of such radiation, for McClennan and Burton have recently shown that the ionisation of a gas inside a closed vessel is diminished by immersing the vessel in a large tank full of water, suggesting that part, at any rate, of the ionisation of the gas is due to a radiation which could penetrate the walls of the vessel, but which was stopped to an appreciable extent by the water. To explain the heating effect observed with radium, the absorption of this radiation by radium must be on an altogether different scale from its absorption by other metals. As no direct experiments have been made on radium, it is possible that this may be the case; it is not, however, what we should expect from the experiments which have been made on the absorption of this radiation by other metals, for these experiments have shown that the absorption depends solely upon the density of the absorbing substance, and not upon its chemical nature or physical state; if this law hold for radium, the absorption by it would be on the same scale as the absorption by lead or gold, and altogether too small to explain the observed effects. We are thus led to seek for some other explanation. I think that the absence of change in the radium has been assumed without sufficient justification; all that the experiments justify us in concluding is that the rate of change is not sufficiently rapid to be appreciable in a few months. There is, on the other hand, very strong evidence that the substances actually engaged in emitting these radiations can only keep up the process for a short time; then they die out, and the sub-

sequent radiation is due to a different set of radiators. Take, for example, Becquerel's experiment when he precipitated barium from a radio-active solution containing uranium, and found that the radio-activity was transferred to the precipitate, the solution not being radio-active; after a time, however, the radio-active precipitate lost its radio-activity, while the solution of uranium regained its original vigour. The same thing is very strikingly shown by the remarkable and suggestive experiments made by Rutherford and Soddy on thorium; they separated ordinary radio-active thorium into two parts, transferring practically all the radio-activity to a body called by them thorium X, the mass of which was infinitesimal in comparison with that of the original thorium; the thorium X thus separated lost in a few days its radio-activity, while the original thorium in the same time again became radio-active. This seems as clear a proof as we could wish for that the radio-activity of a given set of molecules is not permanent. The same want of permanence is shown by the radio-active emanations from thorium and radium, and by the induced radio-activity exhibited by bodies which have been negatively electrified and exposed to these emanations or to the open air; in all these cases the radio-activity ceases after a few days. I have recently found that the water from deep wells in Cambridge contains a radio-active gas, and that this gas, after being liberated from the water, gradually loses its radio-activity; the radio-activity of polonium, too, is known not to be permanent.

The view that seems to me to be suggested by these results is that the atom of radium is not stable under all conditions, and that among the large number of atoms contained in any specimen of radium, there are a few which are in the condition in which stability ceases, and which pass into some other configuration, giving out as they do so a large quantity of energy. I may, perhaps, make my meaning clearer by considering a hypothetical case. Suppose that the atoms of a gas X become unstable when they possess an amount of kinetic energy 100 times, say, the average kinetic energy of the atoms at the temperature of the room. There would, according to the Maxwell-Boltzmann law of distribution, always be a few atoms in the gas possessing this amount of kinetic energy; these would by hypothesis break up; if in doing so they gave out a large amount of energy in the form of Becquerel radiation, the gas would be radio-active, and would continue to be so until all its atoms had passed through the phase in which they possessed enough energy to make them unstable; if this energy were 100 times the average energy it would probably take hundreds of thousands of years before the radio-activity of the gas was sensibly diminished. Now in the case of radium, just as in the gas, the atoms are not all in identical physical circumstances, and if there is any law of distribution like the Maxwell-Boltzmann law, there will, on the above hypothesis, be a very slow transformation of the atoms accompanied by a liberation of energy. In the hypothetical case we have taken the possession of a certain amount of kinetic energy as the criterion for instability; the argument will apply if any other test is taken.

It may be objected to this explanation that if the rate at which the atoms are being transformed is very slow, the energy liberated by the transformation of a given number of atoms must be very much greater than that set free when the same number of atoms are concerned in any known chemical combination. It must be remembered, however, that the changes contemplated on this hypothesis are of a different kind from those occurring in ordinary chemical combination. The changes we are considering are changes in the configuration of the atom, and it is possible that changes of this kind may be accompanied by the liberation of very large quantities of energy. Thus, taking the atomic weight of radium as 225, if the mass of the atom of radium were due to the presence in it of a large number of corpuscles, each carrying the charge of 3.4×10^{-10} electrostatic units of negative electricity, and if this charge of negative electricity were associated with an equal charge of positive, so as to make the atom electrically neutral, then if these positive and negative charges were separated by a distance of 10^{-8} cm., the intrinsic energy possessed by the atom would be so great that a diminution of it by 1 per cent. would be able to maintain the radiation from radium as measured by Curie for 30,000 years.

Another point to be noted is that the radiation from a concentrated mass of radium may possibly be very much greater than that from the same mass when disseminated through a large volume of pitch blende; for it is possible that the radiation from one atom may tend to put the surrounding atoms in the unstable state; if this were so, more atoms would in a given time pass from the one state to the other if they were placed so as to receive the radiation from their neighbours than if they were disseminated through a matrix which shielded each radium atom from the radiation given out by its neighbours.

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ENTROPY.

The Thermodynamics of Heat Engines. By Sidney A. Reeve, Professor of Steam-Engineering at the Worcester Polytechnic Institute (U.S.A.). Pp. xiv + 316 + 42. (New York: The Macmillan Company; London: Macmillan and Co., Ltd., 1903.) Price 10s. 6d. net.

THIS is a very good specimen of that sort of book which is an amplification of the lecture notes of a professor who has carefully prepared problems for students. We may not always like the way in which he introduces the subject to his pupils, and we may say that it is unphilosophical and even cryptic, and sometimes too brilliant, but such comments are often due to the fact that his way happens not to be the usual way of presenting the subject. The way of Prof. Reeve probably suits his particular class of unscientific pupils very well. He uses terms in senses somewhat different from those in common use. He is absolutely correct in many statements with which we would willingly find as much fault as Macaulay did with those of Robert Montgomery. For example:—

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“The universe is eternal. In the face of its steadfast continuity man's momentary existence and evanescent will are as cloud-wreaths against a mountain side.”

We do not know why it should be thought necessary when an engineer is presenting the usual useful application of the known laws of thermodynamics that he should introduce it in thirty-six pages of this style of writing.

When the author comes to the actual problems which may be worked out by a simple application of the t, ϕ diagram, he is a fairly safe guide to the student, although here and there we should have liked him to point out on what assumptions he is working.

Perhaps readers of NATURE will allow us to give a short description of the way in which even elementary engineering students are now able to solve what used to be considered very difficult problems.

We assume that at any instant a pound of stuff is all at the same pressure p and temperature t , and that it has a volume v . There is some law connecting p , v and t so that any two of these three will define the state of the stuff. During any infinitely small change of state, the stuff gives out mechanical energy or does work $p.dv$ if dv is its change of volume; let it receive the heat energy dH . Stating all energy in the same units, the net gain of energy by the stuff during the change is $dE = dH - p.dv$. This E is called the intrinsic energy of the stuff. We assume that there is no other kind of energy to be given to or taken from the stuff than heat and work. The first law of thermodynamics states that, if stuff is carried through a cyclic change and is brought back to its original state, the integral of dH is equal to the integral of $p.dv$, and E comes back to its original value. The integral of dH is not zero, the integral of the work $p.dv$ is not zero, but the integral of dE is zero. The gain of intrinsic energy in a closed cycle is zero. The second law of thermodynamics is that if we divide dH by t the absolute temperature (on a perfect gas thermometer) of the stuff and call dH/t a gain $d\phi$ of entropy, then the integral of $d\phi$ in a complete cycle is zero.

The mathematical statements of the first and second laws of thermodynamics are, therefore:— E and ϕ are properties of the stuff which are known if the state of the stuff is known. Or, dE and $d\phi$ are complete differentials.

Thus in any state of 1 lb. of stuff we know its

p, v, t, E and ϕ ,

and (except during change from solid to liquid, or liquid to vapour, when p and t are not independent) any two of these five enable all the others to be calculated. Hence, graphically, a diagram showing how any two of them alter, is a diagram which completely defines the changing stuff. This has been known ever since Rankine and Clausius discovered the second law of thermodynamics. It is owing to Mr. McFarlane Gray's persistency in advocating the use of the ϕ, t diagram in conjunction with the p, v diagram that engineering students are able so easily to work problems, especially in stuff which is in the liquid-vapour condition.

Since work is $p.dv$, the area of a p, v diagram re-